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millions annually in New York City. We cannot understand on what he bases that assertion. Take the two cities separately. The Illinois counties of Cook and Lake, that in which Chicago is situated and its neighbor on the north, lost 11,433 inhabitants by death in 1880. Of these, 6,230 were infants under five. If, of the remaining 5,203, 5,000 had died with average estates of \$5,000, 175 with \$100,000, and 28 with \$1,000,000, — a most preposterous assumption, — then in those two counties Mr. Jacobson's tax would have netted \$3,075,000. But the conditions are impossible. It is the same with New York. In 1880, New York and six adjoining counties had 25,239 deaths of persons over five years of age. Making an assumption regarding their estates as preposterous as that made in the case of Chicago, the return from Mr. Jacobson's tax would have been less than \$7,000,000.

Two things are very evident, — first, that Mr. Jacobson made no estimate of what his plan would cost; second, that he very much overestimates the number of fortunes of \$20,000,000 and over, in this country. His tax is 50 per cent on fortunes of \$5,000,000 and over, to be sure; and, if a few persons possessing that sum were to die at once, the return would be far greater than we have estimated. But such persons do not all die at once, and moreover, in the long-run, our overestimate of the number of millionnaires would suffice to make up the sum their deaths would contribute. It might even happen that Mr. Jacobson's estimate of the number of immense fortunes is approximately true: the amount raised by the tax would still be far short of the necessary expenditure. The plan is a brilliant one. It has many excellent points. We admire its author's enthusiasm for the manual-training school. His suggestion as to a graduated tax on estates commends itself to our judgment. But as a plan to solve the labor problem, it will not work. This is partly because the income under the plan would not pay the expenditure, and partly because the labor problem is, in many respects, the problem of human nature. In Mr. Jacobson's sense of the word "solution," it cannot be solved.

NOTES AND NEWS.

THE annual meeting of the Association of the Colleges of Ohio will be held at Athens, Dec. 26, 27, and 28, 1887. The following is a list of the papers expected: Monday, Dec. 26, opening address, by Pres. Eli T. Tappan, commissioner of common schools. Tuesday, Dec. 27, 'The Aim of the College,' by Prof. C. L. Ehrenfeld, Wittenberg College; 'Rhetorical Studies and Literary Work in College,' by Prof. W. B. Chamberlain, Oberlin College; 'The Claims of Classical Archaeology on Classical Teachers,' by Prof. B. Perrin, Adelbert College; 'Geology and Mineralogy in our Colleges,' by Prof. J. F. James, Miami University; Symposium, 'The Elective System with Us, What we Do and What we Think,' by the presidents or other representatives of all the institutions in the association. Wednesday, Dec. 28, 'Preparation for College in Ohio,' by Prof. Charles Chandler, Denison University. Meetings of the association will be held in the chapel of the Ohio University; entertainment at the Central Hotel, at \$1.50 per day, and at the Warren House at \$2 or less, according to the number stopping there. Trains leave Columbus for Athens at 7.45 A.M., 3.10 P.M., and 6.10 P.M., standard time.

— A literary and musical entertainment was given at the residence of Mr. and Mrs. H. Herrman in New York on Wednesday evening, Dec. 7, in aid of the Erminnie A. Smith memorial prize fund at Vassar College. The evening was a very enjoyable one, there being two hundred and fifty persons present, and a fine collation being furnished by Mrs. Herrman.

— The five lessons on problems in physical geography delivered by Prof. W. M. Davis, under the auspices of the Teachers' School of Science of the Boston Society of Natural History, during the winter of 1886-87, were so novel and useful to teachers, that he has been invited to give a course during the coming winter upon the physical geography of the United States. This course will be in part a continuation of last year's lessons; but the addition of new matter, new models, more extended illustrations, and the special attention given to our own country, will make the lectures practically distinct from those given last winter.

LETTERS TO THE EDITOR.

** * Correspondents are requested to be as brief as possible. The writer's name is in all cases required as proof of good faith.
Twenty copies of the number containing his communication will be furnished free to any correspondent on request.
The editor will be glad to publish any queries consonant with the character of the journal.*

Conspiracy of Silence.

THE discussion published in a recent number of your journal (*Science*, x. No. 252), relative to the faith of scientists, is the revival of a topic which seems to have been long since definitely settled. If history can be credited, scientific men in every age have fought vigorously against progress. An interesting example is furnished by a brilliant French novelist, Mr. Paul Féval, and probably few will fail to recognize the truth of the following quotation: —

"Il fallut cependant des années encore pour que ce savant et illustre corps, le marine de l'Etat, voulût bien prendre en considération cette force qui fait reculer le vent et se rit de la violence même des courants. Il est vrai que l'Académie professait, vers le même temps, cette opinion: qu'une vitesse de dix lieues à l'heure, sur un chemin de fer, supprimerait la respiration chez l'homme et tuerait tous les malheureux assez fous pour se livrer à ces folles expériences. Il serait puéril d'accuser notre marine ou nos académies. Le monde est ainsi fait. Tout progrès gêne quelque intérêt ou froisse quelque orgueil.

"Dans le doute, abstiens-toi, disait la sagesse antique; la sagesse moderne répond: *Si tu ne sais pas, empêche!* Fera-t-on jamais le compte des hommes et des idées mis à mort au nom de ce fantôme idiot que les sages nomment *l'invraisemblance?*"

The naïve confession of Mr. Bonney practically concedes the whole case. Here are two theories of the formation of coral reefs, each dependent upon a certain set of facts, accessible to all investigators. Mr. Bonney says that the scientific method is to wait, and not to investigate. He is not able, he says, to make up his mind which theory is correct. Is this really a scientific method?

The ideal scientist, it will readily be admitted, is a person whose sole aim is to discover the truth of any matter under investigation, regardless of all personal or partisan feelings. The actual man of science, for the reason that he is a man, is influenced, unconsciously it may be, by his human characteristics, and frequently allows prejudice to overcome reason. In the particular case already considered in your columns, it appears that Mr. Murray discovered some facts which were unknown to Darwin, and that, these facts admitted, Darwin's theory must necessarily be modified. This is the precise point which Mr. Bonney adroitly evades: does he believe the facts stated by Mr. Murray; and, if so, can he reasonably continue to accept Darwin's theory? What excuse is there for waiting, unless, indeed, Darwin is an idol whose sayings, because they were made by him, must be received with reverence by all his followers?

This theory of Darwin's is only one of a number of beliefs which scientists uphold with obstinacy, in the face of contrary evidence; but, as is well said by the writer already quoted, —

"Mais, en tout siècle, les sages eurent beau se coucher au travers de la grande route où marche l'humanité, l'humanité passa. L'invraisemblance, grotesque épouvantail, recule ses brouillards devant la lumière. Des miracles, déclarés impossibles, se promènent paisiblement dans nos rues. Et tout va vite: voyez! il y a de cela quarante ans à peine; en cherchant bien, vous trouverez certes encore, vivant et grignotant sa brique du budget, quelqu'un de ces Spartiates dont la main tremblotante essaya d'arrêter la vapeur!"

It may be of interest to glance briefly at another celebrated theory, which has been treated by scientists in a manner very similar to that pursued in the case of coral formations. About the year 1844, Messrs. Favre and Silbermann experimented on the heat evolved by the combustion of certain elementary and a few compound combustibles. Their experiments, far surpassing in accuracy all those hitherto made, were accepted by scientific men generally, and their results are given in most text-books and treatises on heat. These distinguished experimenters did not think it necessary to test the heating-power of the familiar compound, coal, but considered that it could be calculated with sufficient accuracy by analyzing the coal, and assuming that the heating-power was the same as the sum of the heating-powers determined for the various combustible elements, less the unavailable heat of so much of the

hydrogen as would unite with oxygen in the formation of water; and this is the theory contained in most modern text-books on combustion prepared for the use of English-speaking students, and generally employed in calculations by the scientific men of England and the United States.

About the year 1860, Messrs. Scheurer-Kestner and Meunier-Dollfus made experiments on the heat evolved by the combustion of various coals; using the same method as that employed by Messrs. Favre and Silbermann, and checking the latter's experiments on wood charcoal and hydrogen gas, before testing the coals. The experiments on coals showed that it was incorrect to calculate the heat of combustion of coal from the heat of its combustible constituents as determined by Favre and Silbermann; or, in other words, that it was not correct to assume that the carbon in coal was of the same density as wood charcoal, and that the hydrogen of the coal was in a gaseous state: these being the necessary assumptions, when Messrs. Favre and Silbermann's constants are used in the formula to which reference has been made above. The report of Messrs. Scheurer-Kestner and Meunier-Dollfus has been well named 'classical,'—all operations and calculations being fully detailed,—so that, speaking rationally or scientifically, the conclusion seems inevitable that a scientific investigator must either find some error or accept the results. Well, how has the scientific world, that is to say, the English-speaking scientific world, received these results? Generally by ignoring them, and going on in the good old way, according to the creed formulated by Messrs. Favre and Silbermann. Here is a scientific (?) statement made by one investigator who has carefully studied the report (Mr. B. F. Isherwood, in *Journal of the Franklin Institute*, July, 1884):—

"The results of the calorimetrical experiments made by Scheurer-Kestner and Meunier-Dollfus on the heat of combustion of the Alsatian coals, were never accepted by the British scientists, notwithstanding that no error was ever pointed out in either the apparatus or the method employed. Nor could the writer ever accept them, although he bestowed the closest scrutiny and study upon them."

This is science, with a vengeance! "I can't find any mistakes in the methods or calculations," says the scientist, "but the results are opposed to my present belief, and I can't accept them. I have published numerous treatises containing calculations founded on the methods and data of Favre and Silbermann, and these new results, which would condemn my work, must be ignored or denied."¹

The statement contained in the above quotation, that the results of the experiments made by Messrs. Scheurer-Kestner and Meunier-Dollfus were never accepted by the British scientists, is not absolutely correct. Mr. John Percy, in the last edition of his treatise on fuel, gives the results, and calls attention to the inaccuracy of the ordinary method of calculation. Similar corrections are made in the last supplement of Watt's 'Dictionary of Chemistry.'

Quite recently, Messrs. Scheurer-Kestner and Meunier-Dollfus have repeated their former experiments, obtaining substantially the same results as before; and it seems probable that right methods of calculating the heat of combustion of coal will be generally adopted before long. If the results are true, they will certainly be accepted, some day; but the length of time during which they have patiently awaited admission to the temple inhabited by English-speaking scientists is a sufficient answer to the question, 'If a new fact, overturning some established theory, is presented, do the scientists examine it critically, and either disprove or accept it, or do they ignore it as long as they can, and only take it into their hearts when worn out by its persistent demands?' If a *truth* is announced, there need be no fear that it will not prevail in the end; but numerous facts, similar to that just cited, sufficiently disprove

¹ Scientific men seem to hunt in couples, so to speak; and Mr. Bonney, in answering the Duke of Argyll (*Nature*, Nov. 24), argues in the same manner as his American brother in the above quotation. He does not accept the new theory, and thinks that no reasons are required for his disbelief. Here are his words:—

"To conclude, the Duke still—and this is our special complaint—treats the matter rather according to ecclesiastical than to scientific method. He is fully persuaded of the excellence of Mr. Murray's hypothesis, and considers it to be 'one of those discoveries in science which are self-luminous,' and 'must carry conviction to all.' Very well, but there are some people, not very few in number, who do not share this opinion."

Hail to the new science, announced by Mr. Bonney! The voice of many people is the voice of God.

the theory fondly entertained by many scientists, that they have reached the ideal state where they desire only to know the truth, regardless of consequences.

Another brilliant French writer, Mr. Alexander Dumas, well sums up the matter, as follows:—

"Il est vrai que peut-être les contemporains ne me croiront pas. . . . Qu'importe! je l'aurais dit; d'autres me croiront: la vérité est une de ces, étoiles qui peuvent rester des mois, des années, des siècles, dans les profondeurs du ciel, mais qui finissent toujours par être découvertes un jour ou l'autre. J'aime mieux être le fou qui se voue à la recherche de ces étoiles-là, que le sage qui salue et qui adore, les uns après les autres, tous ces soleils que nous avons vus se lever, que l'on nous a donnés pour des astres immutables, et qui, à tout prendre, n'ont jamais été que des météores plus ou moins durables, plus ou moins brillants, plus ou moins trompeurs, toujours fatals!"

RICHARD H. BUEL.

New York, Dec. 7.

The 'Act of God' and 'Fuerza Mayor.'

MR. APPLETON MORGAN'S 'Act of God' and Mr. Nevin's 'fuerza mayor' appear to me to be pretty much alike, and to threaten a new peril to railway travel,—a peril, according to Mr. Nevin, which in Mexico is already to be encountered. I tremble to think what might happen, for example, if the engineer of the locomotive should happen to sneeze just as he passed a signal that a bridge had been carried away somewhere on the Mexican Central Railroad by "the flooding of a river." Here would be a double 'fuerza mayor'; for an inclination to sneeze is certainly irresistible, and, besides, "the flooding of a river" certainly relieved from the responsibility for the irresistible inclination, even if, according to Mr. Nevin, it did not relieve the watchman from the duty of putting up the danger-signal. But, although we may have to take our lives in our hands when we travel by rail in Mexico (according to Mr. Nevin), I hope that time has not yet come in the United States.

In short, this is the actual practical answer to Mr. Morgan's cleverly reasoned and delightfully *insouciant* paper. It may not be the answer a railway lawyer would write, or would recognize as sufficient, but, from the travelling public's standpoint, it is all there is to be said. It is all very well for the sleek attorneys of great railroad corporations to say that so long as the company provides, as Mr. Morgan says, "the last improvement in safety-insuring devices," its responsibility for the safety of those it transports ceases. "Let us bow to the Divine Will, gentlemen of the jury," says Mr. Morgan. "An overruling Providence has decreed that my client should "roast thirty-two human beings in slow agony on a floor of ice at White River. But our track was in perfect order, our engine was all right, we were running on time. We are not legally to blame." Would Mr. Appleton Morgan have bowed to the Divine Will if he had happened to have been rescued in a half-roasted condition at White River, less an arm, or an eye, or a leg? I venture to say he would have done nothing of the sort. I venture to say he would have commenced proceedings against the company for twenty-five thousand or fifty thousand dollars as soon as he could swear to a complaint. And yet Mr. Morgan will concede that the accident at White River could not have happened in spite of the Divine Will.

The people of this nation do not exist at the will and pleasure of the railway-companies; nor is this nation governed by Mexican laws. Mr. Morgan's familiarity with his subject enables him to write very plausibly concerning the rights and duties of railway-companies; but he cannot convince me, for one, that they are not more sinning than sinned against. If the principle of the 'Act of God' is to be resurrected in the United States, as in Mexico, where is the line to be drawn, and who is to draw it,—the railway-companies, or their ingenious lawyers?

GEORGE BRADWIN.

Jersey City, Dec. 6.

The Flight of Birds.

My friend, Prof. Frank H. Storer, has called my attention to an important note on the wings of birds, by that accurate and indefatigable investigator, Prof. Jeffries Wyman. It is to be found on p. 169, vol. v., *Proceedings of the Boston Natural History Society*. This note is all too short, but forms an interesting adjunct to the